That is to say, although the length of the albino was only $4\frac{1}{2}$ per cent. less than that of the normal hake, the deficiency in girth amounted to 11 per cent. and the deficiency in weight to 23 per cent.

The question arises whether the emaciation of body, and lack of pigmentation, should be regarded as results of some disease (which was not otherwise apparent); or whether the lean condition should be attributed to the insufficient nutrition of a predaceous fish whose stalking powers had been reduced by its conspicuous appearance.

The hake is a predaceous and nocturnal fish, which preys on mackerel, herring and other active fish, especially at night.

The bulk of evidence appears to favour the view that albinism in fishes is a congenital, and not an acquired character (*cf.* colour variation in flat fishes); and I am not aware that leanness of body is specially correlated with the albino condition.

Perhaps some of your readers could refer me to other records which would throw light on this case?

Plymouth, October 10. WALTER GARSTANG.

Tenacity of Life of the Albatross.

SIR WILLIAM CORRY told me some time ago that on one of his steamships coming from New Zealand, an albatross, supposed to have been choked dead, kept in an ice box at a temperature which was always much below freezing point, was found to be alive at the end of fourteen days. He has been kind enough to obtain for me the following statement in writing from Captain Reed. Of course the birds mentioned in this statement could not really have been choked dead, but I think the facts are very interesting. JOHN PERRY.

October 11.

THE bird referred to was supposed to be killed by being strangled with twine tied as tightly as possible round the neck. This twine was not removed. The beak was closed and tied and the legs crossed behind the tail and tied. It was then wrapped in an old meat cloth and put with three other birds in the return box at the end of the port snow trunk. It remained there for certainly not less than ten days at a temperature of from zero when machine blowing on that side to 18° F. when blowing on the starboard side. The snowboy complained that the bird "grunted" when he went near it with his lamp, and Mr. Coombes, the 1st Ref. Eng. brought it out. When put down on the engine-room floor, it could move its neck about and open its beak, and the eyes were open and lifelike. The lower half of the body and the legs were frozen hard. The fastening on the beak had come off. It was alive for two hours after being taken out, and was then strangled and put in the snowbox.

There was another bird treated in the same way, and hung up by the beak in the meat chamber for over four days, and then found to be alive and able to make a "grunting" noise. The temperature of the chamber was never higher than 4° F., and often 8° to 10° below zero. Mr. Coombes, then 1st Ref. Eng., now in *Star of Australia*, and Mr. Boyes, then 2nd, now 1st Ref. Eng., both declare this to be quite true.

If opportunity offers on the passage home I will try how long it is possible for these birds to live in these low temperatures,

S.S. Star of New Zealand, Wellington, WM. J. REED.
August 22.

The Peopling of Australia.

IN the issue of NATURE for October 4, Mr. J. Mathew has questioned the accuracy of certain observations upon the linguistic part of his book, "Eaglehawk and Crow," which were made by me at the request of Prof. A. C. Haddon, and included by him in the review of Mr. Mathew's book in NATURE for December 28, 1899.

I shall be glad if you will permit me to reply as briefly as possible to the complaints in Mr. Mathew's letter.

Mr. Mathew charges me with being "unnecessarily caustic" in my remarks on his theories, and with attending to "petty points" instead of the main issues. To the former charge I must plead zeal for accuracy, and fear of the formation of hasty conclusions. To the second I may be allowed to say, that as the whole of Mr. Mathew's theory (linguistically at least) is based upon the "petty points," their accuracy is vital to the whole structure. Although on p. 44 of his book Mr. Mathew disavows the fallacy that "likeness of words in sound and meaning is a proof of a common origin," he nevertheless adopts it in very many of his comparisons. Take, for example, the Malay and Central Australian words on p. 59; the south-west Australian and New Guinea words on p. 72, the examples in his chapter on the Malay element in Australian, and the satisfaction expressed in his letter to NATURE at a comparison between Australian, Malay and New Hebridean, because the "terms for father and skin are the same."

My summary of his chapter on the Malay element in Australian is quoted by Mr. Mathew in his letter as "ridiculous nonsense." I maintain that it is a perfectly fair summary of his actual words. He states on p. 5 that "Malay refers generally to the people of that race to the north of Australia without distinguishing nationality," and on page 61 that the Malay invasion came from the north. Speaking of the invaders, he says on page 61, "The straggling stream winds about here and there, touches the shore at various places and recoils back inwards," but when I state that the meaning of this is "wandering about the interior," he says the latter phrase is a "pure invention of Mr. Ray's."

Although Mr. Mathew declares in his letter that the Malays came from an indeterminable (though probably Sumatran) locality, all the Malay words in his proofs are those of the current literary or colloquial Malay, and several of them (*e.g.* tangan, gigi, kapala, bapa, rambut), are by no means the common words used by the Malayan peoples of the Archipelago. In two instances his words are incorrect: *kaka* is wrongly given *kaku* (p. 59), so as to agree with Australian words like *kako*, *kahkooja*, and '*duwan*' (p. 60), said to mean 'ear' is probably meant for *daun*, "leaf," which only means the "external ear," *i.e.* the 'leaf of the ear,' when conjoined with *telinga*.

That Mr. Mathew believes the Malay words were "scattered all over the island continent" plainly appears from his examples. He shows so-called Malay words on the coast of New South Wales, East Queensland, and the extreme east (p. 58); others across the centre of Australia from the Gulf of Carpentaria southward, and on Cloncurry River (p. 59), and others in West Australia (p. 60).

Mr. Mathew states that in the *Journal* of the Anthropological Institute for 1894–5, I have used languages as the basis of a classification of the New Guinea Islanders. That is so, but my method is not comparable with Mr. Mathew's. I showed that certain New Guinea languages (Motu, Keapara, &c.) should be called Melanesian because they agreed with the languages of the Melanesian islands, *almost entirely in grammar, and very largely in vocabulary*, and that others should be called non-Melanesian because they had *no agreements whatever* with the Melanesian. Can Mr. Mathew show by a similar grammatical and lexical comparison, that the Australian is related to any other group of languages? With regard to terms like 'bapa' and 'mama' for 'father' and 'mother,' my argument was that no dependence can be placed on these words to show a connection of languages. They are among the earliest vocables uttered by a human being, and in very many languages of the world have become appropriated to the earliest recognised human relationships.

This is not the time or place to reply to the somewhat contradictory propositions in Mr. Mathew's letter. He wishes me to prove: (1) That words of 'mama' type are not adopted words in Malay; (2) that they were not earlier in use in the East Indian Archipelago; (3) that they are not more markedly Papuan than the 'bapa' type. I may, however, be permitted to remark: (1) That words for father containing the syllable *ma* (of which *mama* is a reduplication) are the commonest in the vocabularies of the tribes of Borneo, Celebes, Philippines, &c., least subject to Malay influence, whilst words containing the syllable *ba* or *pa* are confined to the nearest connections of the Malay. Hence the words of *ma* (or *mama*) type are the original, not adopted words, and (2) were necessarily the earlier in use. Mr. Mathew's second proposition thus contradicts his first. Also (3), the languages of the Papuans in West New Guinea have forms of *bapa* for 'father,' those in Central New Guinea have *babe* or *apai*. One Papuan and all the Melanesian have forms of *ma* (*ama* or *tama*).

Mr. Mathew complains that I have not explained the New Guinea numerals. Could I do this within the limits of a review? The convergence of Australian forms towards Cape York, stated by Mr. Mathew, does not necessarily imply that the words came from New Guinea, and his examples only show that the Saibai

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numeral *may be* connected with the Australian. On the opposite New Guinea coast the numerals for 'one' are very different to the Saibai *urapon.* They are: *nambi* (Morehead River), *ambior* (Wasi Kasa), *tarangesa* (Bugilai Tribe), *iepa* (Kunini Tribe), *atanok* (Dabu Tribe), *netat* (Murray Island), *nao* (Kiwai Island), *monou* (Purari Delta), *farakeka* (Papuan Gulf), *aia* (Yule Island). Mr. Mathew asks us to believe that the Kalkadoon numeral "*luadi*" (two) is a Melanesian numeral used in Australia 150 miles south of the Gulf of Carpentaria, when the only comparable form in Melanesia is the Duke of York Island *ruadi* (second). This is a flagrant example of the method adopted, though disavowed, by Mr. Mathew.

The point missed by Mr. Mathew in discussing the phrase "*ori kaiza,*" which I called mongrel, is that he has no proof that '*ori*' and '*uroi*' are the same words. Considering that the Gulf tribes have a word (*uiva*) for 'cassowary' (the emu is not found), and that nowhere else in New Guinea is there a word similar to *ori*, meaning 'bird,' and also that it requires a Torres Straits word to give '*ori*' an Australian meaning, it is highly improbable that it can explain words like *waitch, wadgie, warritch* for "emu." The Saibai for 'big bird' is '*koi urui,*' for 'cassowary,' '*samu.*'

In his letter, Mr. Mathew objects to my calling comparisons of Australian with Malay and New Hebridean "absurd and misleading." He suggests, without any warrant, that I find the absurdity in analogies between Malay and New Hebridean, whereas I have directly affirmed the connection of Malay with Melanesian (including New Hebridean) in the *Journal* of the Polynesian Society for 1896. The real absurdity is that of supposing that there is a relationship between Australian, Malay and New Hebridean, because "the terms for father, skin are the same."

In conclusion, I must again express my regret that Mr. Mathew should regard my criticism of his work as 'mere fault-finding and ridicule.' I have studied these languages for many years (without postulating theories), and have much material yet unpublished. To point out the weakness of Mr. Mathew's argument, with regard to method and deductions is fair criticism, and should not lead to a charge of unsoundness and inaccurate knowledge. If, as Mr. Mathew states, his materials are imperfect, why found a theory upon them?

218, Balfour Road, Ilford, Essex, SIDNEY H. RAY.
October 14.

RECENT AND PROPOSED GEODETIC MEASUREMENTS.

IN the history of geodesy and the discussion of the problem of the figure of the earth, the measurement of three arcs of meridian stands out conspicuously. These are the Peruvian and Lapland arcs in the Northern Hemisphere, and that of Lacaille in the neighbourhood of the Cape of Good Hope. Those who took part in the original measures worthily distinguished themselves, but it is inevitable that with the demand for greater accuracy in geodetic inquiries the necessity for repeating the ancient measures should be acknowledged. Without experience and with imperfect instruments, it is remarkable that the old astronomers accomplished what they did. To determine the approximate figure of the earth, and to derive a fairly accurate value of the compression was no mean achievement. But Lacaille's arc of meridian soon fell into disrepute, and other measures have had to be substituted by Maclear and others. Svanberg repeated the original work of Clairant and his colleagues, in Lapland, soon after its completion, and within these last few years a new determination of the Peruvian arc has been imperatively called for. There is no doubt but that the French nation, who have so honourably distinguished themselves in the difficult task of geodetic measurement, will be able to undertake this work, and increase their scientific reputation. From 1734, when the enlightened Government of the day undertook the measurement of two of the aforementioned arcs, down to our own time, when we have seen the Mediterranean successfully bridged by a geodetic survey, and the interior of Africa connected in one unbroken chain with our own Shetland Islands, French men of science have played a conspicuous part in all questions connected with the true figure of the earth. At a moment therefore when the re-measurement of the famous arc of Peru has been forced upon us, it would have been with a feeling akin to disappointment if we did not find the French nation eager to repeat the historic work with all the skill that long experience has suggested, and all the accuracy that modern science demands. The report of a committee of the French Academy of Sciences, however, assures us that the ardour displayed by the French in the past is no whit abated, and that though the sacrifice of time and money is considerable, these drawbacks will be cheerfully borne. For the arc in Lapland another has been substituted on the Island of Spitsbergen, and the necessary work of triangulation has been for some time quietly carried on under the auspices of the Russian and Swedish Governments; while at the same time, the reports of Her Majesty's Astronomer at the Cape of Good Hope tell us what is being done in the way of supplementing Lacaille's arc in the Southern Hemisphere. With this activity before us, it seems a fitting opportunity to compare the aims and the motives that inspired those who inaugurated the earliest geodetic expeditions with those that will influence and guide the latest surveys in the same or similar regions.

In the middle of the last century a distinct issue was presented to the scientific mind. Then the general figure of the earth was an undetermined problem, and whether the polar or the equatorial axis was the longer was a vexed subject of controversy. It is amusing enough now to read of the disputes between the partisans of defective observations on the one hand, and the upholders of an incomplete theory on the other. To-day no such broad issue is before us, the differences are of a more subtle character, demanding great nicety of observation and more effective analysis. Then the true difficulties of the problem were scarcely apprehended. Only a few years earlier, Fernel had attempted to determine the length of a degree by counting the number of revolutions of his carriage-wheel between Paris and Amiens, a method which recalls the earliest attempts of Eratosthenes. Picard, it is true, had recognised the necessity of employing means of greater accuracy, and had taught the true principles of geodetic measurement; but the methods pursued by his descendants, and the precautions taken to ensure accuracy and recognise the surface to which the measurements are referred, constitute almost a new science. For with accumulating materials and greater experience, it has become necessary to distinguish between three surfaces. First, the ellipsoid of revolution, which corresponds most nearly to the form of the earth; secondly, the true geoid, that is to say, the surface of equilibrium of water at rest under the influence of centrifugal force, and the attraction of the earth's mass; and, thirdly, a corrected geoid, differing but slightly from the true, in which it is attempted to eliminate the effect of unequal masses on the earth's surface and in its interior. Theory shows that the true length of the arc of meridian, measured on the corrected geoid, will be given in terms of the measured base, if the effect of local attractions has been correctly determined. It is precisely in overcoming the difficulty of correctly eliminating the effect of local attractions, and of reducing the length of the measured base to the level of the water surface of the geoid, that the measurements of this century will mainly differ from those of the last.

A preliminary survey of the district undertaken by Captains Maurain and Lacombe has shown that the country of Peru possesses peculiar difficulties for an accurate geodetic survey. The levels vary very considerably with the distance from the coast, while here and there mountains of a volcanic character rise to a height of 6000 metres. But for the interest attaching to a historic site,

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